

Music." The book itself may without unfairness be described as an "arrangement," or rather as a "pot-pourri," inasmuch as it resembles those musical compositions in which some of the fragmentary themes of one or more great masters are dished up for the public in some new or popularised setting, consisting of commonplaces of a more or less florid type. About 80 per cent. of the pages before us consist of clippings and quotations taken *verbatim et literatim* (and in quotation marks be it added) from the works of Helmholtz, Stone, Pole, Tyndall, and Sedley Taylor, interspersed with a connective-tissue woven from the "author's" own brain. We have found this ingenious fabric very remarkable reading, and have gleaned a number of new facts from it. We have learned, for example, that the transmission of verbal messages, prayers, hymns, and sermons through the telegraph wire by the telephone must be held to "prove that air is not the only medium through which sound-impulses can pass." We find our author declaring on p. 80 that the reason why so romantic a name as the "siren" should be applied to so matter-of-fact an instrument *does not appear*; while on p. 98 he seems to have made the discovery that the name is a misnomer, because "Homer's *Σειρῖππος*" (*sic*) were not endowed with the power of singing under water as this instrument can. Our author is very unhappy in dealing with equal temperament, and complains that nearly all writers on temperament, with the exception of Mr. Ellis, describe it as dividing the octave into twelve precisely equal semitones, "without explaining that these semitones are not absolutely equal." That the perfect equality of the theoretically equal temperament is never attained *in practice* is indeed true; but why does our author find fault with writers on temperament for stating the exact theory? His accusation against Dr. Stone for palpable misuse of language (on p. 359) is utterly out of place, and only shows that the author has not comprehended the true meaning of a musical interval as defined by a ratio. He appears not to know that if an octave is divided into twelve exactly equal geometrical parts or ratios, the differences between the successive terms of the ratios are not, and cannot be, arithmetically equal to one another. Hence his attack on the perfectly unexceptionable statements of Dr. Pole and Dr. Stone. The diagrams with which the work is interspersed consist principally in pictures of syrens and in copies of wave-forms taken from Mr. Sedley Taylor's "Sound and Music," and spoiled by drawing them as if made up of semicircles pieced together. The wave-form given on p. 266 to illustrate beats does not show the wave-form of the beat *at all*: and though the author gives on p. 102 a wave-form which illustrates a beat admirably, he appears not to know it, as he passes it by as being merely one of a few different forms of tracing which a vibroscope can register. But we have said enough to justify us in having at the outset pronounced "The Student's Helmholtz" to be what we called it—a *pot-pourri*—or, in the plain English tongue, a *hash*.

Afrika im Lichte unserer Tage. Bodengestalt und geologischer Bau. (With a Hypsometrical Map.) By Josef Chavanne. (Vienna: A. Hartleben.)

THE conclusions come to by Herr Chavanne we have already referred to. Africa, he finds, is, on the whole, a high plateau or table-land, crossed here and there by mountain-chains or single elevations. The plateau commences in most places at a remarkably short distance from the sea, the slopes south of the equator being particularly steep. North of the equator the land may be looked upon as a very slightly inclined plane, which, like the southern plateau, is also crossed by separate elevations, some of them being very considerable. The presence of numerous, and for the greater part widely-distributed, lakes is unlike the general physiognomy of the other large continents. By far the most important

part of the author's work is the excellent hypsometrical map which accompanies the book, and to which we referred a short time ago. Its scale is 1:30,000,000. The elevations are marked in eight different tints of brown, showing so many grades and altitudes from zero upwards. Thus at one glance we see the African continent rising as a rule from 0 to 600 metres in the northern half, while, in the southern half, elevations from 900-1200 metres are the rule. The greatest heights—those of 1500-2000 metres and more—are packed close together on the east side, between the southern end of the Red Sea and the Zambesi River, and only occur again in the extreme south-east (Natal) and far up in the north-west (Atlas). The text of the book is well written; the author's descriptions are always attractive, to the point, and free from all superfluous wordiness.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Struggle of Parts in the Organism

THE review of Dr. Roux's work on the "Struggle of Parts in the Organism" by Mr. Geo. J. Romanes which appears in your number of September 29 (p. 505) contains some passages which, I venture to think, are hardly consistent with the purpose to which the columns of NATURE are devoted. I understand that purpose to be the discussion of scientific facts and scientific laws, properly so called. I should be the last to deny that these facts and these laws may have, and indeed must have, their own ultimate bearing upon theology, whether natural or revealed. But it is not the purpose of a purely scientific journal to enter upon this discussion; it is one which cannot be there pursued without involving controversies alien to the spirit in which physical science ought to be studied and explained.

And if even temperate discussion upon the subject ought to be avoided in a purely scientific journal, still more ought there to be a scrupulous abstention from dogmatic utterances which are hostile to theological opinions, and which are unsupported by even the semblance of argument.

In the passages to which I refer Mr. Romanes asserts that to the whole "argument from design" in nature an "end has come"—as the result of Mr. Darwin's Theory of Evolution—that the "fountains of this great deep have been broken up by the power of one man," and that "never in the history of thought has a change been effected of a comparable magnitude and importance."

As an expression of the opinion of Mr. Romanes that the Darwinian theory ought to put an end to the "argument from design," this assertion may be allowed to pass. But as the assertion of a fact I venture to say that it has no foundation. There are many minds, including some of the most distinguished in science, who not only fail to see any contradiction between evolution and design, but who hold that the doctrine of evolution and the facts on which it is founded have supplied richer illustrations than were ever before accessible of the operations of design in nature.

I should be transgressing my own rule were I to defend this view in your columns. I shall therefore content myself with saying that no possible amount of discovery concerning the physical causes of natural phenomena can affect the argument that the combination and co-ordination of these causes which produce the "apparent" effects of purpose are really and truly what they seem to be—the work of Mind and Will.

Inverary, October 4

ARGYLL

Solar Chemistry

THE researches of Mr. Lockyer, and others, summarised by him in recent numbers of NATURE, have to a great extent complicated the aspect of this grand problem, which appeared so

simple to Stokes and Thomson in 1852, and to Stewart and Kirchhoff a few years later.

I wish to consider briefly, what are these new and puzzling complications of the solar problem; and whether we may not still preserve our belief in the existence of *essentially different* elementary atoms, which is the basis of the beautiful Vortex Theory. For it seems that to hazard (however *naturally*) such a step as is involved in assumed dissociation of the (so-called) elements, before we make certain that no less serious hypothesis will account for the observed facts, is contrary to the spirit of Newton's *Regule Philosophandi*.

The most prominent of these complications seem to be—

(1). The variations of the relative brightness, width, &c., of the lines in the spectrum of a particular substance, in dependence on the source and circumstances of its incandescence.

(2). The so-called "long" and "short" lines. (These, as will be seen, are probably a case of (1).)

(3). The fact that, in the spectra of sun-spots, *some* lines supposed to be due to a particular element indicate rapid motion of the glowing gas; while others, supposed due to the same element, give no such indication.

(4). The (at least apparent) coincidence of lines in the spectra of two or more elementary substances.

To these may be added:—

(5). The remarkable peculiarities of star-spectra; especially the paucity, and the breadth, of the lines in the spectra of *white* stars.

As regards (1), let us consider a sounding body with a large number of different modes of vibration, exposed to impacts either periodic or at least with an average period. The relative intensities of the various notes which it can give will obviously depend upon the period of the impacts. Now this is precisely the case of a particle (I use the word to avoid misconception) of a glowing gas. The average number of blows it receives will depend on (a) the number of particles per cubic inch (and also upon *whether there be another gas present or no*, a point of very great importance) and (b) the temperature, which is directly connected with the velocity of the particles.

Change the density, the temperature, the admixture with foreign substances, or any two, or all, of these; and the *average period of the battering* to which a particle is subjected may be so altered as to elicit from it *in any required ratios of relative intensity* the various simple rays it can give out.

It will readily be seen that this may account for all of the phenomena of classes (1) and (2) above.

(3) may be accounted for in many ways. I mention only one, as my object is merely to show that we are *not yet* compelled to accept dissociation of so-called elements even in its mildest form. Other modes of escape, though not quite so simple, present themselves.

What is seen in a sun-spot is the integral, as it were, of all that is taking place (as regards both radiation and absorption) in many thousand miles of solar atmosphere, containing the same substance under the most varied conditions. That portions in which certain lines of that substance are prominent over others may be at rest relatively to the observer along the line of sight; while others, in which (from different density, temperature, or admixture, as above explained) other lines are specially prominent, may have large relative velocities, is certain. This would at once account for these singular observations.

As to (5) we must remember that in a star spectrum we have, as it were, a *triple* integral. For we not only integrate through the depth of the atmosphere, but also over the whole surface of the star; spots, hurricanes, and rotation of the whole, included. This is equivalent to the *superposition* of innumerable separate spectra, no two of which may have *any one* individual line in the same place or of the same breadth, &c. Feeble lines may, in fact, entirely disappear under such treatment.

(4) If not due to want of dispersive power in the apparatus, this may be legitimately attributed to inevitable impurities. It is only in "tall talk" (or in advertisements) that any human preparation, elementary or not, can be spoken of as "*chemisch rein*." And we all know how faint a trace of impurity can be detected by the help of the spectroscope.

Even in the last resort, I see nothing to hinder the existence of exactly equal vibration-periods in two perfectly distinct vortex-atoms:—though their occurrence is extremely improbable.

If we could get an absolutely transparent gas; one, therefore, which could give no radiation under any circumstances; the study of the behaviour of a given quantity of hydrogen mixed with dif-

ferent proportions of it in a vessel of given size, and subjected always to the same conditions of incandescence, would give us invaluable information.

G. H.

Replacing Flakes on Palæolithic Implements

THIS wonderful feat was first performed by my friend Mr. F. C. J. Spurrell of Dartford. On first thoughts the thing seems utterly impossible, and it is obvious that no flake can possibly be replaced upon an implement unless one lights on the exact spot where the instrument was made, and finds both implement and flakes in position. Mr. Spurrell so found his material. During the present summer I have discovered another and similar Palæolithic floor, far removed from Mr. Spurrell's, and where implements and flakes are exposed in a stratum perfectly undisturbed since they were gently covered up in Palæolithic times with fine sand containing the shells of such freshwater molluscs as *Unio*, *Cyrenia*, and *Bythinia*. For obvious reasons—the chief one of which is that my work would be totally stopped if I mentioned the locality—I will content myself with stating that the position is nearly a mile from any river, and the floor is 41 feet above the level of the nearest stream; above the floor is a thick deposit of fine stratified sand, and above that loam. On this Palæolithic floor I have found several implements and a large number of flakes, and on one of the finest implements, an example 6 inches long, $3\frac{1}{2}$ inches wide, and weighing $1\frac{1}{4}$ lb. I have been able to replace two flakes, one $2\frac{1}{4}$ inches long, the other 2 inches in exact position; the flakes slightly overlap each other on the implement, and both have been struck from the edge of the implement at right angles across its face. The implement and flakes were close together, and with them I found a hammer-stone of flint with a distinctly battered and abraded edge. Mr. Spurrell replaced many flakes round his implement, but the implement itself was a spoilt and poor example. My implement, on the contrary, is an unusually fine one, large, heavy, and perfect. Both the implement and flakes show a little of the original grey crust of the flint from which the instrument was made, and this peculiar grey colour led me to attempt the replacement of the flakes with the above-mentioned successful result. One flake has a slightly uneven edge—in some instances considered a proof of use—the second flake is quite sharp. I shall exhibit this implement, with other implements, flakes, &c., from the same place, at an early meeting of the Anthropological Institute. WORTHINGTON G. SMITH

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Integrating Anemometer

PERHAPS the following brief description of the integrator devised by me will suffice to establish its near kinship with Mr. Wilson's (*NATURE*, vol. xxiv. pp. 467 and 557):—A roller with a spherical edge is made to revolve with a velocity proportional to that of the wind as recorded on an anemogram. This roller presses on a plane table carried by two mutually perpendicular pairs of rails in planes parallel to that of the table. The lowest of the pairs of rails is supported by a frame carried on the extremity of a vertical shaft. The point of contact of the roller with the table lies in the prolongation of the axis of the shaft. The table can rotate with the shaft, but not independently. By a simple arrangement the shaft, and consequently the table, are caused to take up positions corresponding from moment to moment with the direction of the wind record on the anemogram. A style concentric with the shaft presses lightly against a compound sheet of tracing and carbonised paper attached to the under side of the table. Arrangements are also made for obtaining the sum of the movements of the table toward each of the four cardinal points. If the roller be moved with a velocity proportional to that of the wind, whether directly by a cup-anemometer or by a mechanical translation of the trace as given by such an instrument, while the table simultaneously assumes orientations corresponding to the direction of movement of the air, the line drawn by the style will be a miniature copy of the path of an imaginary particle animated by the movements actually belonging to the masses of air which successively affect the anemometer at the given station during the selected period, rigorously in accordance with the principle known as Lambert's. But in order that the trace drawn as described should correctly represent the actual movements of the air, it is evident that the whole mass of the atmosphere must be supposed to move "parallel to itself," *i.e.* in such a manner that the straight